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CHOLERA.

Notes on the Pathology and Treatment of Cholera. By GEORGE JOHNSON, M. D., F. R. C. P., Professor of Medicine in King's College. (Continued from p. 53.)

The General Principles of Treatment.—The account which I have now given of the pathology of cholera, although brief, will, I trust, have been sufficient to convey to those who have carefully read these papers a definite idea of the doctrine which I advocate. If there are any who desire to have further details, and proofs, and illustrations, these may be found in a book which was published ten years ago, but which has hitherto attracted singularly little notice.

I proceed now to the inquiry, What has this pathology to do with treatment? And I maintain that it is a compass which will guide the intelligent practitioner over a "sea of troubles" and perplexities in the treatment of cholera. We now have a definite view of the nature of cholera, of the relation which the symptoms bear to each other, and of the manner in which the disease tends to a spontaneous recovery. We can see that, amongst a vast number of plans of treatment, some have been unquestionably beneficial by assisting the curative efforts of Nature, while others have been as unquestionably detrimental by opposing those efforts. Others, again, have been of a mixed character; as, for instance, the combination of calomel—a purgative—with opium. And, lastly, some methods have been simply neutral in their effects, except in so far as the knowledge that something was being done for him may have inspired the patient with hope and confidence. As an illustration of this, I may mention one of the many infallible cures which have found their way into the *Times*. I mean the inoculation with quassia. If that which has been satirically said contained the whole truth—namely, that "the

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chief art of the physician is to amuse the patient while Nature performs the cure"—the inoculation with quassia would be as effectual as any other method of treatment, and it would have the great negative merit of doing no harm.

There is no remedy which has the slightest pretension to be considered a cure for cholera; no drug or agent which, so far as we know, will neutralize the poison or lessen its virulence. I have not the faintest hope or expectation that a specific remedy for such a disease as cholera will ever be discovered. The number of really specific remedies which we possess for any diseases is unfortunately very small. Those who believe in specifics are, in general, ignorant of the nature of disease and of the true methods of cure; and this ignorance renders them quite incompetent to estimate the influence of treatment.

If I have correctly interpreted the phenomena of cholera, the natural method of cure is eliminative. The process of elimination takes place by two successive stages. 1. There is excretion from the blood into the stomach and bowels; and 2. The morbid contents of these viscera are ejected by vomiting and purging.[1] Can we do anything to assist this natural curative process? Unquestionably we can; by emetics and purgatives. But is it necessary or beneficial to interfere in any way? Ought we not rather to leave the cure entirely to Nature? I have no doubt that much may be done with advantage. And my conviction of this is based partly upon theoretical grounds, and partly upon the actual results of treatment. If we carefully observe the condition of a patient in collapse, we shall often find that the intestines are more or less distended with fluid; and this, too, while perhaps there is general torpor and very little effort at expulsion. Again, it is often found that, although there has been copious watery purging during life, the small intestines contain after death a large amount of a peculiar viscid dirty white material, having a very offensive odour. A purgative may be useful in removing both these conditions; namely, over-distension of the bowel by liquid, and accumulation of offensive viscid semi-solid secretions. The object of a purgative in cholera is not to increase excretion from the blood into the stomach and bowels; but simply to assist in the expulsion of the morbid secretions from the digestive canal. To fulfil this indication, that purgative is best adapted which acts most speedily, yet with the least amount of irritation. I believe that castor oil best accomplishes this object; but a great variety of purgatives have been given with more or less success.

When, in September, 1854, I first publicly stated that I was giving castor oil as a purgative in cholera, the announcement was received with a howl of execration. Give a purgative to a patient whose blood has already been drained of all its liquid and thus rendered too thick to circulate! Unheard of atrocity! Statistics must immediately be published to show that this plan is most deadly in its effects.[2] But at the same time statistics were showing that calomel in frequent and sometimes in large doses is a very successful remedy. How does the calomel act? Is it not an irritant and a violent purgative? Or what is there to deprive it of its purgative properties when given for the cure of cholera?

Calomel a purgative! exclaims indignant orthodoxy. No! calomel is an alterative. It acts upon the liver, and restores the secretion of bile. Those who give this explanation of the action of mercury in cholera, are of course quite unaware what is the real cause of the suspended secretion of bile during collapse, and are therefore equally ignorant of the conditions essential for the restoration of that secretion. To call calomel an alterative and a restorer of the biliary secretion does not, like its com-

[1] If there are any who doubt the elimination of a *morbid poison* by the stomach and bowels, no one can doubt the existence of offensive *morbid secretions* which require to be cast out.

[2] It was evidently the opinion of some who chanced to be in prominent positions at that time, that my alarming outbreak of heterodoxy must be treated, like cholera, by very active repressive measures. So fierce an onslaught was made upon me, that I began to fear lest what Sydenham said of smallpox two hundred years ago, would be found equally applicable to cholera in the present century. "I venture to assert," said Sydenham, "that the physician who has much to do with smallpox runs many risks with his reputation. The vulgar are ever in the habit of ascribing deaths to the officiousness of the attendant; whilst physicians themselves catch greedily at opportunities for slander. They make out their case before incompetent judges, and procure most uncharitable verdicts. They act thus in order that they may build up a name for themselves upon the ruined reputation of others; a proceeding disgraceful to even honest artisans—doubly disgraceful to scholars." (The *Works of Sydenham*, translated by Dr. R. G. Latham for the Sydenham Society, vol. i. p. 137.)

bination with opium, deprive it of its purgative properties. I therefore claim the reported success of the calomel treatment as evidence in favour of elimination.

Saline Treatment.—Another plan of treatment which has unquestionably been attended with a large amount of success is that which is known as the saline treatment of Dr. Stevens. The theory upon which this treatment is based—namely, that the essential cause of cholera is a deficiency of the saline constituents of the blood—has been shown by accurate chemical analysis to be fanciful and erroneous; but this happens to be one of those rare cases in which a theory may be wrong and the practice based on it not far from right. I have condensed from Dr. Stevens' work (*Observations on the Nature and the Treatment of Asiatic Cholera*) the following account of his theory and his practice.

"The first efforts towards a cure should be directed to assist nature in throwing off the poison from the blood, *per vias naturales*, and, at the same time, supplying the necessary saline stimuli, on which the action of the heart and its vessels depends. In accordance with this view, patients presenting merely the first stage of premonitory symptoms, diarrhœa and vomiting, took, on their admission, a Seidlitz powder: and, if sinking was felt but without bowel complaint, more active purgatives were then employed; or three or four teaspoonfuls of Epsom salts were added to the Seidlitz powder. On the bowels being moved, plenty of thin beef-tea, well seasoned with salt, was given. This simple treatment was so successful, that a great many patients had no further complaint, and were generally dismissed cured in a few days.

"If cramps, coldness, or sinking of the pulse, were present, the patients were considered as cholera patients in the second stage. The 'non-purgative' salts were administered every half hour, or more or less frequently, according to the symptoms, and in the following dose: Muriate of soda ℈j; carbonate of soda ℈ss; chlorate of potash gr. vij. When life seemed rapidly ebbing, the collapse stage having been reached, a strong solution of the same salts, at a temperature of 100°, was thrown into the bowel. The saline mixture was administered half hourly; and, in severe cases, the muriate of soda was increased to a drachm, or even more, as circumstances seemed to require. In some cases, an enema composed of a large tablespoonful of common salt in water, at as high a temperature as the patient could bear, was administered every two or three hours."

This plan of treatment has been attended with a large amount of success in the hands of many different practitioners; but, for a very obvious reason, it has not been generally adopted. Its mode of action is essentially eliminative; and, therefore, it is incompatible with that theory of collapse through loss of water which has taken such firm hold of the European medical mind.

This theory of collapse, which now, we trust, has received its death-blow, started into life when cholera first visited Europe in 1832. Before that time, our countrymen in India could not only *give* purgatives as a part of the regular treatment of cholera, but they might also *call* them purgatives without subjecting themselves to the imputation of culpable rashness and folly. Abundant illustrations of the use of purgatives in the treatment of cholera may be found in the writings of the following Indian practitioners: Bell, Orton, Scot, Curtis, Twining, Christie, Rogers, Kennedy, Searle, and Corbyn. None of these writers had any very definite pathological theory. They looked upon cholera as a disease chiefly of the nervous system. They called it spasmodic cholera; and they often gave opium to relieve cramps at the same time that they gave very active purgatives to remove the morbid secretions from the bowels. The fact that, in many instances, their treatment was a mixed treatment—a combination of opiates with purgatives—renders their writings the more instructive, since it clearly appears, on a careful study of their cases, that, in proportion as either the opiate or the purgative plan prevailed, was the mortality increased or diminished.

Several of these authors speak highly of the use of castor oil, on account of its unirritating properties, and the completeness with which it brings away the viscid and putrid secretions from the bowels.

The very interesting paper by Mr. Watkins (*British Medical Journal*, Oct. 28, p. 445), to which I have before referred, contains a most instructive illustration of the effect of various modes of treatment. It will be seen that four distinct sets of cases are there described. In one set opium was

given alone; these were all fatal. In a second set, the opium was combined with calomel or with gray powder; of these more than half died. The purgative action of the mercury somewhat lessened the mortal effects of the opium. In a third set of five, calomel alone was given; and of these one died, while four recovered. In a fourth set of twenty-one, castor oil was given; and nineteen of these patients recovered. Besides these, one case which had been treated by gray powder and opium passed into collapse, and then, castor oil being substituted for the other remedies, she ultimately recovered. Lastly, one most interesting case in full collapse was treated by castor oil until he rallied; then, through some mistake, two pills, probably of calomel and opium, were given, he fell again into collapse; he was a second time treated by castor oil and ultimately recovered.

It is sometimes argued, that no remedies can possibly be of use during collapse because absorption is suspended. Now, in the first place, Magendie has shown that absorption is not absolutely suspended during collapse, though it is rendered much less active. He injected camphor into the rectum, and detected its odour in the breath in five minutes, whereas in health, he says, it may be detected in one minute. He performed the same experiment with ether, and with a similar result. He reasonably infers that absorption is slowly carried on during collapse. (*Leçons sur le Cholera Morbus*, p. 97.) There is undoubted evidence that opium and alcohol become absorbed, and produce narcotic effects during collapse. But the remedies which are of real use during collapse have no need to be absorbed. Surely a mustard or salt and water emetic need not be absorbed in order to excite the stomach to expel its contents. And it can scarcely be doubted that calomel and castor oil, by their stimulant action on the inner surface of the bowel, may excite muscular contraction, and so act as purgatives without being taken up by the blood-vessels.

A writer on cholera during the last epidemic stated that "brandy, ether, and ammonia, turpentine, and even champagne, were given to rouse the flagging energies of life, but to little purpose; they were generally thrown from the stomach as soon as swallowed, and draughts containing creasote, chloroform, and camphor, shared the same fate. The stomach had, as it were, lost the power of minister[illegible]g to the wants of the system."

Now, it may be ask[illegible] whether the stomach was not much [illegible]iser and more useful than the doctor, and whether it could better "minister to the wants of the system," than by the expulsion of its morbid secretions, together with such a horrible mixture of drugs as could not be retained and absorbed without adding to the perilous oppression of the vital powers.

During the last epidemic, I observed very carefully the effect of *brandy* in the few cases in which I ventured, in a tentative manner, to give it to patients who were in collapse. I found that it speedily increased the restlessness and feeling of oppression; at the same time the pulse lost power and volume, and the temperature fell. I infer, therefore, that brandy and all alcoholic stimulants during the collapse stage of cholera add to the obstruction of the pulmonary circulation, instead of passing freely through the lungs and then stimulating the left ventricle, as they do in other states of system. I resolved, therefore, never again to give alcoholic stimulants in collapse, no matter how great the *apparent* exhaustion may be.

In a few cases, the *sesquicarbonate of ammonia* was given with apparent benefit; and this remedy appears to deserve a further trial as a stimulant during the stage of collapse.

It is worse than useless to attempt to *feed* a patient during collapse. The secretions of the stomach are utterly deranged, and the power of digestion is suspended. The mildest nourishment administered at this time only adds to the feeling of oppression and general distress, from which the act of vomiting often gives immediate relief. Upon this point, I have satisfied myself by repeated and very careful observation.

The wife of the English chaplain in Paris has recently obtained notoriety by administering *chlorodyne* in fifty or sixty cases of incipient cholera, and, as it is stated, "has succeeded in arresting the disease in every instance." What would be the effect of chlorodyne in cases of choleraic diarrhœa? It would relieve the cramps, and, by its narcotic operation, it would in some degree retard the spontaneous recovery of the patient. Nevertheless, if a lady have a determination to step beyond her own proper

sphere, and to play at doctoring, chlorodyne would be a much less dangerous drug in her hands than opium and strong astringents.

Dr. Buller and Dr. Risdon Bennett have recently spoken in high terms of the good effects of *external warmth* to the skin of patients in collapse. Dr. Buller recommends the hot mustard bath; and Dr. Bennett packing in hot wet blankets. There can be no question that to thoroughly warm a patient in collapse is often a real benefit; the pulse, the temperature and the color of the skin, and the expression of the features, all improve simultaneously. And we can readily understand this effect of warmth if we have thoroughly comprehended the *modus operandi* of hot saline injection into the veins. But too much must not be expected from the application of external warmth. The warm bath has often been found to have a very depressing effect. Annesley declares that, in his opinion, it did more harm than good. "The fatigue arising from going in and coming out of it, and from rubbing and dressing the patient, exhausted him." (*Diseases of India*, p. 156.) Christie found the bath injurious; and Dr. Parkes says: "I have seen a man walk firmly to the bath, with a pulse of tolerable volume, and a cool but not cold surface, and in five or ten minutes have seen the same man carried from the bath, with a pulse almost imperceptible, and a cold clammy skin. I cannot find in my notes a single case in which the warm bath appeared beneficial." (Parkes *On Cholera*, p. 210.)

During the epidemic of 1849, I saw several patients in King's College Hospital placed in the hot-air bath; and the result of my observation was that, while the warmth appeared to be beneficial, by relieving the cramps, and sometimes even improving the pulse, yet, on the whole, the patients appeared to be rather distressed than comforted by the bath. The employment of hot baths in the treatment of cholera obviously requires caution. Faintness is a not uncommon result of remaining too long in a hot bath, even when the bather is in health; and, in the collapse of cholera, a sudden and deadly exhaustion may be thus induced. Then be it remembered, that choleraic collapse is a form of asphyxia. There is an instinctive craving for air. Again and again will the patient toss all covering from his body; and it is likely that surrounding the body with hot water, or hot air, or wet blankets, may cause distress by interfering with cutaneous respiration—*i. e.*, the aeration of the blood through the skin. A very convenient way of keeping up the warmth of the body is by the application of hot bottles, hot dry flannels, or hot sand-bags, to various parts of the surface.

Dr. Murray, an Indian practitioner, obtained very striking results in the improvement of the pulse by *injecting into the rectum* every half hour, or hour, or two hours, according to the urgency of the case, a pint of a weak solution of common salt and carbonate of soda, at a temperature of 120°. (Rogers' *Reports on Asiatic Cholera*, p. 241.) The injections were retained generally from two to four minutes—sufficiently long to impart considerable warmth to the blood in the vessels of the large intestine; and to this, without doubt, the good effects were due. It appears to me that hot injections into the rectum may be used with advantage in cases of extreme collapse.

The good effects of heat are unquestionable. But what is to be said of *ice* or *iced water*, either taken into the stomach, or applied externally? During collapse there is usually a sensation of burning in the region of the stomach, and a strong craving for cold liquids. Iced water relieves that sensation of heat, and it has a powerful influence in checking vomiting. Are these sufficient reasons for giving iced water to a patient in collapse? In my opinion, decidedly not. I am sure that vomiting, when not excessive, is beneficial; and I believe that iced water lessens the burning sensation in the stomach by diminishing the vascularity of the mucous membrane, and thus interfering with the excretion of the morbid poison, upon the ejection of which by vomiting depends the patient's recovery. For these reasons, I believe that the administration of iced water to a patient in collapse is injurious. In most cases, I would allow an unlimited quantity of water of the temperature of the room; but, in extreme collapse, I would persuade the patient to drink *hot liquids*, with a view to add some warmth to the blood.

The application of *ice to the spine* has been recommended by Dr. Chapman. The theory which suggested this practice is a speculative web spun from the projector's brain. There is no evidence to show that

this practice has been useful in any case. That some patients subjected to this distressing mode of treatment would recover, is certain; it is equally certain that their chances of recovery would be lessened by the depressing influence of the ice on the nervous system, and its chilling effect on the blood. We know that, if we can thoroughly warm a patient in collapse, we improve his general condition; and this, whether the warming be effected by the external application of heat, or by hot injections into the veins or into the rectum. We therefore can have no difficulty in perceiving that to thoroughly *chill* a patient in collapse must be a painful and a dangerous proceeding.

Venesection, as we have seen, has often afforded great relief during the stage of collapse; and I have before pointed out that the only rational interpretation of its *modus operandi* is that, by lessening the over-distension of the right cavities of the heart, it increases the contractile power of their muscular walls. The symptom which appears especially to call for venesection, and which has most commonly been relieved by it, is rapid breathing, with an oppressive sense of suffocation. When, with these symptoms, there is a cessation of vomiting and purging, which is probably a result of the almost entire arrest of the blood in the lungs, I believe that venesection affords the only hope of benefit. If during the last epidemic I had known as much of the essential cause of collapse, and of the influence of venesection, as I now do, I believe that I might have saved more than one patient who, for the want of that knowledge, was lost.

In this communication, I have done little more than sketch the general principles of treatment. The only safe guide in conducting the details of treatment is to have a very clear and definite view of the pathological interpretation of symptoms, and of the mode in which remedies may assist the natural process of cure. Without this pathological knowledge, it is scarcely possible to avoid falling into some of the many errors which have affected the treatment of cholera, and which have often led to the neutralization of the good effects of one remedy by the simultaneous use of other means having an opposite and an injurious influence.—*British Med. Journ.*, Nov. 25, 1865.

President Lankester's Address in the Public Health Department of the Social Science Association.—(Continued from page 57.)

"I will not now attempt to settle the question whether these diseases are contagious or not; whether they arise from a special poison which is propagated in one body and is conveyed to another, or whether there are certain general conditions of external agents which engender poisons capable of spontaneously producing them. I need but generally remind you that amongst our sanitary reformers there are two great schools, the disciples of one of which maintain that the great mass of zymotic diseases are produced by special poisons, and are called contagionists; whilst the followers of the other school do not believe in the existence of special poisons, but maintain that certain general conditions of sanitary neglect and dirt are alone necessary to produce the group of zymotic diseases, and they are now called anti-contagionists. Now, I believe that the extreme views of each school are wrong; and I have a very deep impression that for sanitary measures to be directed by one or other party, in the present state of our knowledge of the diseases, would be to plunge us into worse evils than quiet submission to their unresisted influence. For an officer of health to suppose that cleansing, and draining, and washing, would arrest the progress of smallpox in a house full of unvaccinated persons, would be an utter absurdity; whilst the placing a cordon around an ill-ventilated and badly warmed house, expecting to keep off bronchitis and pneumonia, whilst the temperature is 12° below freezing point, would be equally absurd. But, whilst all are agreed that smallpox is a contagious disease and bronchitis is not, there is a large class of diseases on which sanitary authorities differ as to their nature, and the best modes of arresting their development. It is on this account that I now propose briefly to examine what is really known of the nature of contagious diseases and their mode of propagation; and if I succeed in nothing further, I hope I shall be able to show that it is of the utmost importance in all our sanitary operations that we should at least consider the issues of both theories; that, whilst believing in contagion, we should act as if all depended on the removal of the

general external agencies of disease; or, whilst believing in the spontaneous origin of diseases in dirt, we should yet do all to avert the possibility of their propagation by contagion. It is of the highest importance, at the same time, that we should pursue the inquiry into the origin and nature of those zymotic or pneumatic diseases which carry off annually upwards of 100,000 of our population. In order to do this, I think there are three circumstances that demand our attention. There is, first, the poison that is supposed to kindle the disease; and, secondly, there is the medium that conveys it to, thirdly, the person predisposed to take the disease. If we lose sight of any one of these elements in investigating zymotic diseases, we shall most assuredly go wrong, and practically commit great mistakes. Thus, let me take smallpox as an example. In order to propagate this disease, there must be, first, the poison-matter from a smallpox pustule; secondly, a medium of conveyance, either the point of a lancet or an atmosphere to convey the poisonous germs; and, thirdly, there must be a person predisposed to take it. If the poison be not there, no amount of predisposition, that we are aware of, will engender the disease. Again, if the poison be there, and the predisposed person, there must be a medium of conveyance; if a predisposed individual be at one end of a room and the affected individual at the other, and the current of the atmosphere blow from the unaffected to the affected individual, no poison will pass and no disease be established. Or, again, the atmosphere may be so extensive as to dilute the poison to a tenuity by which it becomes powerless; or the atmosphere may be artificially ozonized, or iodinized, or chlorinized, so as to destroy the germs of the poison. But let the poison be ever so intense, and the medium ever so ready to convey it, if the unaffected individual have had the smallpox or have been vaccinated, no disease will be produced. Hence, we must study the poison-makers, the poison-bearers, and the poison-takers.

"Now with regard to poisons, even the anti-contagionists admit that, in what they call miasmatic diseases, conglomerations of dirt and filth, or matters in their wrong places, do enter the system, and, at one time or another, do upset and damage the healthy working of the machinery; and they are also bound to state in a scientific way what compounds are produced by dirt, and in precisely what way they affect the system. There can be no doubt that certain inorganic agencies, such as carbonic acid, sulphuretted, phosphuretted, and carbonated hydrogens, ammonia, and sulphocyanogen, do produce injurious effects upon the system. Continued exposure to such exhalations may prevent a proper oxidation of the tissues, and render the body predisposed to take in the diseases of special poisons; but we have no evidence to show that any of these agents—although they will destroy life—are capable of producing alone any of the forms of miasmatic disease.

"Again, it seems demonstrated that there arise, during the decomposition of vegetable and animal matters, certain organic molecules, which being taken into the system, will produce certain definite changes in the system constituting well known forms of disease. Thus, ague and kindred fevers, called paludal, and paroxysmal fevers, do not seem to be produced by poisons formed in the animal system, but by poisons formed during the decomposition of vegetable matter. A certain amount, also, of the diarrhœa of summer is to be set down to the decomposition of animal and vegetable matters. Certain quantities of these matters are directly taken into the stomach and bowels; whilst others appear to come into contact with the mucous surfaces by inhalation. There is, also, one of the endemic and epidemic fevers of our country that is supposed by high sanitary and medical authorities to originate in the spontaneous decomposition of organic matters in drains and sewers; hence it has been called 'drain-fever.' It is, however, generally better known by the name of gastric or typhoid fever. That this disease is generated by a specific poison, has been demonstrated by Dr. William Budd, of Bristol; and, should it be capable of demonstration that this disease is really generated *de novo* by the matter of drains and sewers, it would be an interesting fact, as showing the possibility of a contagious disease being produced afresh. But, up to the present time, we have no conclusive experiment with regard to the origin of any of the specific contagious diseases. There is no error, perhaps, of more vital importance to the public health than that which was fallen into by some of our early sanitary reform-

ers—a belief in the spontaneous origin of the several forms of diseases produced by specific contagions.

"The most common forms of contagious disease in this country are smallpox, scarlet fever, measles, whooping-cough, typhus fever, typhoid fever, and our occasional visitant, Asiatic cholera. Of the intimate nature of the poisons producing these diseases, we know but little; but recent researches with the microscope lead us to hope that we are not far distant from the time when, at least, the form of these poisons will be made visible to the human eye. It is a fact known to all, that the blood contains two sorts of cells or globules, one red, the other white. The white cells are composed of matter in a state of vital change. It is these cells which accumulate in inflamed parts, and which form the pus found in vesicles, pustules, abscesses, and inflamed surfaces of the body. These pus-cells have a great power of multiplication; and they retain their vitality after they have been removed from the living body. We can convey common pus-cells from one living body to another, and make them increase. There is a disease of the eye attended with a large formation of these pus-cells, and these may be conveyed through the air from one person's eye to that of another, and produce the same disease. In the disease known as pyæmia, these cells assume a specific character; and Dr. Richardson informs me that he has succeeded in producing, artificially, pyæmia in animals by introducing the secretions of an animal affected with pyæmia into one that is not. In smallpox, we have a disease characterized by pustules over the body. Each pustule contains a secretion abounding with pus-cells, and the matter of which they are formed. It is the introduction of this purulent matter into the blood, that sets up the dreadful malady of smallpox. In the same way, we find the vesicle of cowpox charged with white cells, and the 'germinal matter' of Dr. Beale. The germinal matter here, however, does not possess the vitality and energy of that of the smallpox pustule. It is seldom conveyed through the air; like some other animal poisons with which we are acquainted, it requires contact; but, on being introduced into the blood of another person, it produces the same disease—always the same disease, never another. We may learn much of the nature of these poison-cells by the study of those which we know so well. These cells, or germinal-elements, retain their vitality long after they have been removed from the body, if you exclude them from the air. The vaccine lymph has been conveyed between pieces of glass, or dried on tips of bone or threads of linen and cotton, all over the globe, and has been found capable of engendering the disease cowpox. This shows us how all these poisons may be covered over in linen, cotton, and woollen fabrics, how they can be conveyed in letters and newspapers, how they may adhere to inanimate substances of all kinds, and only need the awakening influence of a little moisture to summon them to awake and live anew.

"No one, that I am aware, has yet isolated the poison of scarlet fever, of measles, of typhus, of cholera, and of the other diseases of the group of contagious diseases; but, reasoning from analogy, and there could hardly be a better instance of the process, we are driven to the conclusion that these diseases depend on a cause similar to that of smallpox, and that the real form of the poison is the charged white cell of the blood. It is, however, interesting to notice some varieties in the habits of these poisons. Thus, we are not aware that the poisons of smallpox, measles, scarlet fever, or typhus, are conveyed by any means but through air; whilst there is every reason to believe that the poisons of typhoid fever and cholera are conveyed by the agency of water. There is an interesting relation between this fact and the seat of the disease; for, whilst one set of diseases manifest themselves by eruptions upon the skin, the other set is characterized by derangements of the mucous membrane of the intestines.

"A question of high interest arises here, and it is one that has not at present been settled; and that is, as to whether the poison-matter of these diseases is capable of multiplying itself by cell-division, or the ordinary forms of the growth of fluvial matter out of the body. It does not seem impossible that this should be the case, although at present we have no demonstration of the fact. We know that such multiplication takes place among the lower forms of plants, as the yeast, or ferment-

fungus (*Saccharomyces Cerevisiæ*), and that it also occurs amongst the vibriones and monadinæ in the animal kingdom.

"Another interesting question connected with these poisons, is the possibility of transformation or development. In the animal kingdom, we have this phenomenon taking place, that an animal passing from its egg to its adult state is capable at each stage of multiplying itself, so that a number of creatures are produced at each period of its growth, capable of attaining, under proper external circumstances, the adult stage. Now it may be that, amongst these cell-poisons, there are stages of development at each of which the cell is capable of propagating its own form and no other, till it meets with the proper external circumstances for a further change or development. Professor Huxley relates that, on board the *Rattlesnake*, after they had been six weeks at sea, the cook got an attack of erysipelas; this spread through the ship and ceased. After this, one of the sailors had mumps, and this also spread through the ship. There are other interesting facts bearing on this point; but I throw out these hints here as subjects full of practical importance to the sanitarian.

"Let me now say a few words with regard to the means of conveyance of poisons. The most obvious of these means are the atmosphere and water. The air, to carry most poisons, must be warm and moist. The poison of yellow fever spreads in hot climates; the poison of typhus is arrested by heat on the one side and by cold on the other. It prevails, in fact, only in climates having a range of temperature between 40° and 62° Fahrenheit. At the temperature of boiling water, all these poisons are destroyed. A most important fact, as we have in every household in Europe the means of destroying them.

"But there are other means of conveyance besides air and water. Articles of food, articles of wearing apparel, bed-clothes, curtains, carpets, and all vessels and depositories containing the secretions of human beings, may retain the poison-cells in all their integrity. These things may carry the poisons from household to household, disseminate them in our streets, our omnibuses, our railways, steam-ships, and public conveyances of all kinds. The drain may carry them into the sewer, and the sewer into the river; but, in their course, they may escape from our ventilating-shafts, our gully-holes, and open closets. They may be emptied into our wells and rivers, and conveyed to our bodies by means of spring-water or river-water. They may be shut up in drawers or in old closets (of which there are many striking instances on record), and, at any moment when brought into contact with the human system, they may start into life and activity again, to renew their ravages on systems predisposed to their action.

"That the poison of a disease may exist, and every possible access to a system be present, and yet the disease be not taken, is well known. Instances frequently happen of persons living to old age, without having had any of the ordinary contagious diseases of mankind. It would be well to make a more accurate study of these cases. But we know something of the laws of the predisposition to disease. We know, for instance, that persons who have had the smallpox are not disposed to take it again. We know that, in nine cases out of ten, if persons have been vaccinated, they will not take smallpox. This is one of the great triumphs of our modern civilization. It is the beacon of our hopes with regard to crushing out for ever the poisons that can only be propagated in human systems. But our knowledge of predisposition extends further than this. We know that, where the four great factors of our life have been scantily supplied or vitiated, there predisposition exists, not only to idiopathic and self-generated disease, but to receive the germs of the poison fevers of which I have been speaking.

"So well is this known, that we can point out certain external conditions, which will so act upon the human system as to predispose it to certain forms of disease. Thus, amongst our working men and women, who live in badly ventilated and overcrowded houses, we find those who are most ready to take the poison of typhus. Amongst the underfed, the fever called relapsing, which differs from typhus and typhoid, finds its most ready victims. Hence it has been called famine-fever. But no class or condition of men have been discovered who are not susceptible of these poisons. The medical man who lives free from the influence of overcrowding or famine, in his own home is frequently stricken down with these fevers. The

anxiety of the student, the statesman, or the prince, may undermine his health, and render him a ready victim for the poison that lies concealed like a snake in the grass in his path. It is not necessarily among the overcrowded and badly ventilated rooms of the poor, and the squalor and filth of our lowest classes, that scarlet fever selects its victims and commits the greatest ravages. The homes of the rich and the hearths of the comfortable middle classes of England are made desolate by this all-pervading scourge; and it is a mockery to say that we know the conditions of those who will be the subjects or the victims either of scarlet fever or its twin-sister of mischief, diphtheria. That both these diseases depend on a special poison, which can be communicated through the medium of the air, and preserved in activity on inanimate substances, as clothes and excreta, I have no doubt; but we cannot predict with any certainty who will be their victims."

MEDICAL NEWS.

DOMESTIC INTELLIGENCE.

Approach of Cholera to our Shores.—The steam-ship England, of the National Steam Navigation Company's line, from Liverpool to New York, put into Halifax on the 9th of April, cholera having broken out among her passengers six days previously. She had twelve hundred and two passengers and a crew of one hundred men. No cholera existed at Liverpool at the time of her sailing. The disease broke out among the German passengers, and at the time of her arrival at Halifax one hundred and sixty cases had occurred and fifty deaths. She was put into quarantine, the disease soon disappeared, and she was then allowed to pursue her voyage to New York, where she is now at quarantine.

On the 18th of April the steam ship Virginia, belonging to the same line, arrived at New York with ten hundred and eighty passengers, most of whom were Germans, who had only been a day or two at Liverpool before embarking. Eight days after leaving Liverpool one man who had had slight diarrhœa from the time of sailing, suddenly grew worse and died. On the day of his death (12th April) two other passengers were attacked, and afterwards the epidemic extended until over a hundred cases had occurred before her arrival, of which number forty-seven have died. The whole number of deaths up to the latest accounts is said to reach a hundred and fifty; and the number of new cases is largely on the increase. It is stated, and this is worthy of note, though the same has been before observed, that the disease broke out on board the Virginia when she was in the same latitude as the England was when the disease appeared on that ship.

The ship has been detained at quarantine and every measure taken to cleanse the vessel to make the passengers comfortable and prevent the spread of the disease so far as quarantine measures can effect that object.

Trichinæ in Pork raised in the United States.—A committee was appointed some time since by the Chicago Academy of Sciences to ascertain, first, whether Trichinæ actually exist in the hogs of this country, and in those of the Northwest in particular; and secondly, should they exist, to determine the extent of the danger thereby incurred, and to ascertain the best means of averting it. For the attainment of the first object portions of muscles from 1394 hogs in the different packing houses and butcher shops of Chicago have been examined, and the results presented in tables.

These tables show that the committee "have found trichinæ in the muscles of twenty-eight hogs out of 1394 examined. We may therefore conclude that in the hogs brought to Chicago one in fifty is affected with trichiniasis in a greater or less degree. We must confess our surprise at arriving at this result, which indicates, with little doubt, the startling fact that trichiniasis in pork is even more common in this country than in Germany, where it caused so much suffering and death. For instance, in the city of Brunswick, where a most careful inspection of 19,747 hogs was made in the years 1864-5, only two were found to contain trichinæ in their muscles, the proportion being 1 to 10,000 against 1 to 50, as before stated, in our country. The comparative immunity from disease which our own people have enjoyed, undoubtedly results from our habit of cooking meat before eating it, while in Germany it is eaten raw by the poorer classes on account of the high price of fuel."

The committee have found the spinal muscles more liable to be infested with the trichinæ than other muscles.

The committee point out the following as the means of guarding against the evil :—

"Now that the existence of trichinæ in our pork has been established beyond a doubt, it will be proper for us to point out all known means of defence against its ravages. First, with regard to the rearing of hogs. These animals undoubtedly become infested through the eating of flesh of some kind, since no trichinæ, nor germs of trichinæ, have ever been found in any vegetable food. A strict attention to the feeding of hogs, and their confinement in pens where no animal food is accessible is an infallible preventive against trichiniasis in them. Such management is all the more necessary, since European authorities agree that it is impossible to diagnose the disease in the animal from external appearances, and no culpability can therefore attach to the farmer for selling hogs which prove to be affected with trichinæ.

"In regard to pork, the use of the microscope is primarily indicated. With this instrument only can we ascertain with certainty whether the muscles of the hog are free from the parasite. The general use of the instrument is, however, impracticable, unless a system of microscopic inspection be adopted here, as in Europe, at the great packing establishments. But we have in our power much more simple means of insuring safety in the consumption of pork. It is simply necessary to cook it thoroughly, so that every portion of the meat shall have experienced a temperature of at least 106 degrees Fahrenheit. We cannot insist too strongly upon this point. Again, by properly salting and smoking the meat for a period of at least ten days, the trichinæ, should they exist, will be certainly killed. Simple desiccation of the meat, if continued for a period of sufficient length, will also kill them. They will never be found alive in old hams, for instance. On the other hand, mere pickling appears to have very little effect upon these worms."

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Medical Society of the State of Pennsylvania.—Owing to the difficulty of securing adequate accommodations in the town of Kingston, which was selected for the next annual meeting of the Medical Society of the State of Pennsylvania, and in accordance with the wishes and request of the Luzerne County Medical Society, the meeting of the State Society will be held on Wednesday the 13th day of June next, at Wilkesbarre, the county seat of Luzerne County, and only one mile distant from Kingston.

Delegates upon arriving at Wilkesbarre are requested to report immediately at the office of the Wyoming Valley Hotel, where the committee of reception will be in attendance.

Efforts are making to secure commutation tickets over the various railroads leading from different points in the State to Wilkesbarre.

By order of the committee of arrangements.

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Medical College of Ohio.—At the annual commencement, held on the 1st of March last, the degree of M. D. was conferred on 67 candidates.

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St. Louis Medical College.—At the annual commencement, held on the 2d March last, the degree of M. D. was conferred on 50 candidates.

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Miami Medical College.—(Cincinnati.)—At the annual commencement, held on the 1st of March last, the degree of M. D. was conferred on 26 candidates. The number of matriculants during the session was 154.

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Medical College of the State of South Carolina.—At the session of 1865-6, the number of matriculants was 34, and at its close the degree of M. D. was conferred on 13 candidates.

The next course of lectures in this institution, which has been in successful operation for upwards of 40 years, will commence Nov. 5, 1866.

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Legal Liability in Cases of Surgical Malpractice, in its Principles and Limits.—By Geo. C. Blackman, M. D., Prof. Surg. in Med. College, Ohio, and Stanley Mathews, Esq., of the Cincinnati Bar. This is the title of a work announced as in preparation, and which is promised to be published early in the coming autumn. The numerous vexatious trials to which our profession have been subjected for alleged malpractice, render a complete collection

and digest of cases very desirable to both the medical and legal professions.

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New Medical Journals.—We have again to add to our list of new Journals. Since the rush to California after the discovery of the gold placers there, nothing like the present rush into medical journalism has occurred, and we can only express the hope that the science of medicine may be explored with the same productive energy as were the gold mines.

The first we have to notice is the *Southern Medical and Surgical Journal*, the publication of which was suspended during the rebellion, and which is announced to be revived under the editorship of Dr. Joseph Jones. No. 1 of the third series is to appear on the first of July next. It is to be a bi-monthly.

The second is the *Nashville Journal of Medicine and Surgery*, the first No. of which is to appear on the 1st of July next, and to be continued monthly thereafter. The editor and proprietor is Dr. W. K. Boling.

Third. The *New Orleans Medical and Surgical Journal* is to be renewed under the editorship of Drs. Chaillé and Nichols, and a number is announced to appear in July next.

Lastly we have received the announcement of the *New Orleans Medical Record*, the first No. of which is to be issued on the 1st of June, and semi-monthly thereafter. This is the fourth New Orleans Journal.

We shall be happy to extend to all these journals the usual editorial courtesies, and to place them on our exchange list.

FOREIGN INTELLIGENCE.

Dr. Bence Jones' Discovery of a Quinine-like Substance in the Animal Body.—At the last Friday evening meeting of the Royal Institution, Dr. Bence Jones gave a singularly lucid and interesting account of one of those discoveries which, from time to time, open up entirely new fields of research. It seems that Dr. Bence Jones some months ago desired to detect the presence of quinine in the animal body. For this purpose a test was essential, and the test he adopted was that peculiar influence (fluorescence) of this alkaloid on the refraction of light, whereby it makes the dark part of the spectrum beyond the violet ray luminous. Having fixed on this as a test, and prepared standard solutions of quinine so that he might be able to get something like a notion of quantity, by comparing the effects of solutions of various strengths on the spectrum, he set to work on a guinea pig—gave the creature three grains of quinine, then tested the blood, and crystalline lens, and other parts, and obtained the most satisfactory evidence of the presence of quinine. Luckily, however, for science (though, as may be imagined, it was a source of great perplexity at the time), Dr. Bence Jones, in order to complete the experiment, submitted the same parts of a guinea pig, which had taken no quinine, to the same test, and out came the same reaction; every part of the *non-quinised* animal—heart, liver, kidney, and lens—when treated like bark, in such a way as to dissolve out and purify any alkaloid that might be in them, gave the same spectral reactions as similar solutions did from the animal that had taken quinine. Here was a puzzle! Undismayed, however, Dr. Bence Jones set to work to make out the mystery. He soon convinced himself that the tissues of all animals contain a something which, if dissolved out like quinine, gives the same fluorescent reaction with light. One solution of quinine, interposed in the rays of light, cuts off the fluorescence of another; so does a solution of the quinine-like substance; and so does each upon each interchangeably. Common salt and hydrochloric acid also lessen the fluorescence of both. This quinine-like substance, too, can be demonstrated in the tissues (in the lens) fresh, or preserved in glycerine; and can be demonstrated in the living as well as in the dead. Mr. Bowman had shown it in the eyes of his dog, and Dr. Bence Jones sat down before the electric light, which Professor Tyndall adjusted so as to let the audience see the strange fluorescent gleams from the interior of his eye. It has been found impracticable as yet to isolate this substance, so rapidly does it undergo oxidation; but Dr. Bence Jones had carried out an interesting series of investigations which convinced him that this quinine-like substance was part of the natural constituents of the body; and that judged by the quantitative reactions, the quantity of it was temporarily increased by giving quinine. It was shown by this means that quinine

in a very few minutes after its administration passes into every tissue of the body; that its maximum effect is produced in two or three hours, and then decreases till it disappears in about seventy-two hours. The demonstration of its presence in the crystalline lens gave ground for hope that substances might be found hereafter to remedy perverted nutrition of the non-vascular tissues—as cataract, and even the deposits of gout in cartilages. Then, what a glimpse these investigations afford of the possible *modus operandi* of quinine in the cure of ague! We call quinine a "specific," a term which indicates our notions, or want of notions, of its action, and nothing more. But is it not possible that ague may depend on the absence of a natural quinine-like substance from the tissues? That quinine may supply this want (thus, by the by, showing the futility of all attempts to distinguish between food and physic), and that arsenic, on the other hand, may act within the body as it does without, by preserving certain matters from decomposition? Such is a rough note of this very interesting address, which was as valuable for what it promised for the future as for what it showed at the present.—*Med. Times and Gas.*, March 31, 1866.

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Dr. Richardson's Method of producing Local Anæsthesia.—In the No. of the *American Journal of the Medical Sciences* for April last, p. 512, will be found an account of a very ingenious method devised by Dr. Richardson for the production of local anæsthesia. That method has attracted much attention, and it has been employed in England generally with success. It is stated in the *British Medical Journal* for Feb. 24, "We understand that on Sunday last Mr. Gowland operated on a member of our own profession for fistula, Dr. Richardson himself applying the anæsthetic. The result was so good that the patient, after the operation, was not aware that the cut had been made. On Monday, Mr. Erichsen performed the operation for phimosis, Dr. Richardson again producing anæsthesia; and insensibility was produced in fifteen seconds, and the knife gave no pain whatever. On Wednesday, Mr. W. Adams removed a tumour of the size of a walnut from the foot of a young lady, Dr. Richardson in this case also applying the process. The insensibility was perfect in a minute; and Mr. Adams leisurely dissected out the tumour, without any sensation of pain being experienced by the patient. While the knife was slowly making its way, the lady was asked what she felt. She said she was conscious of the course of the knife, but that it was merely as though something was passing lightly over the surface, although the knife at that time was half an inch deep. On Tuesday, at University College, Mr. Henry Thompson applied the anæsthetic in two cases. He opened an abscess in the breast, and performed eversion of a diseased nail. The ether-jet was applied in each case only a few seconds, and both operations were absolutely painless. Different statements are made by different patients in regard to the sensation produced by the application of the anæsthetic. The lady, from whose foot Mr. Adams removed the tumour, said the sensation was rather pleasant than otherwise; others describe it as slightly unpleasant, and others as burning and sharp. On this point, Dr. Richardson says, a great deal depends on the part of the body subjected, and a great deal on the rapidity of the process. The mucous surfaces, the hands, and the face, are more sensitive than the other parts of the body. More also is felt when the process is prolonged. It is best to push the process on rapidly; and it is good practice, in cases where it can be done, to compress the artery above the part to be operated on, so as to cut off for the time the supply of blood. Other methods for making the process itself quite free of sensation in every case may be devised; and, as Dr. Richardson is now devoting himself closely to experiments bearing on this subject, there need be no anxiety but that this object will soon be accomplished. He is also having a larger apparatus constructed, on what he calls the brush-principle, with the view of applying the method in a capital operation."

In the number of the *Med. Times and Gas.*, for April 7, 1866, Dr. Richardson has published an account of a case of Cæsarean section in which he successfully produced local anæsthesia by his spray apparatus.

This invention of Dr. Richardson's seems to be one of the greatest boons conferred on humanity, for fatal results have sufficiently often followed the inhalation of chloroform to excite anxiety, whenever that method of producing anæsthesia is resorted to.

Animals cannot Live after being Completely Frozen.—M. POUCHET has lately made some careful experiments which tend to show that there is no truth in the statements which have been from time to time made, that certain animals will live after having been completely frozen. He shows that the blood globules are seriously altered by the congelation; and that the blood thus vitiated destroys life when allowed to enter again into circulation. But, in cases of partial congelation, if the blood thus altered be allowed to enter only in small quantities at a time into the current of the circulation, it may be gradually mixed with the blood without destroying life; just as Claude Bernard has shown that a deadly dose of woorara poison introduced into the limb of an animal may, by means of a ligature, be allowed gradually to enter into the circulation, and so as not to destroy life. The first phenomena of congelation observed are, the contraction of the bloodvessels, the consequent exclusion of blood from them, and pallor of the part. Then follow alteration and destruction of the blood globules. If the whole animal be frozen, then the whole mass of blood is altered, and death is the necessary result. When only a small part of the body is frozen, only a small quantity of altered blood passes into the circulation, and consequently life may not be compromised. An animal partially frozen may be kept alive for a long time, if the congelation be maintained; but will perish rapidly, if the frozen part be thawed, and so as to allow the altered and destroyed blood globules, etc., to enter rapidly into the circulation. Hence, in all cases of congelation, life is destroyed not by a stupefying action of the cold on the nervous system, but by the resulting changes effected in the blood by the freezing. The practical conclusion is, that, when a part has been frozen, it should be gradually thawed, and so as not to allow the altered and consequently poisonous blood to pass in too great quantities at a time into the circulation.—*Brit. Med. Journ.*, Jan. 6, 1866.

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Professor Schönbein's Researches on Oxygen.—Various scientific journals having recently announced that Dr. Schönbein, of Basle, had succeeded in decomposing oxygen into ozone and antozone, the French Scientific Association addressed a request to him that he would repeat before it the experiments establishing this discovery. In his reply the celebrated chemist observes:—

"You are perhaps aware that for the last 30 years I have been almost exclusively and uninterruptedly engaged in researches relating to oxygen, and in the course of this long series of investigations I have discovered, concerning this elementary body, a considerable number of new facts, from which I believe I am able to draw the following conclusions: 1. Oxygen may exist in three different allotropic conditions. 2. Two of these states are active, and opposed to each other; I designate them by the names ozone and antozone. 3. Equal quantities of ozone and antozone neutralize each other, forming inactive or neutral oxygen. 4. Neutral oxygen may be separated or transposed, one-half into ozone and one-half into antozone. But it is to be observed that the experimental demonstration of the justice of these conclusions is not as simple as, for example, that of the decomposition of water, and in order to apprehend their logical sequence, it is absolutely essential to become acquainted with a considerable number of facts relating to it. The exhibition of these facts by suitable experiments could not be accomplished at a single demonstration. The journals, moreover, have been in error in announcing that I had succeeded at isolating ozone and antozone in a state of purity. It is true, for a long time past, I have been engaged in attempts in accomplishing so desirable an end, but have not thus far met with complete success. Ozone and antozone are always mingled with neutral oxygen, which depends upon causes intimately connected with the generation of the two active modifications of oxygen. If the Association, after what I have said, continues to wish that I should visit Paris, and if the state of my health allows of my doing so, I shall have great pleasure in delivering a short course of lectures relating to the entire subject of my researches on oxygen."

We need hardly add how acceptable such a course would be from the distinguished discoverer of ozone.—*Med. Times and Gaz.*, March 31, 1866.

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Alcohol from Mulberries.—Many farmers in the south of France not deriving any profit from the leaves of mulberry trees since the disease has prevailed among silkworms,

rooted up the trees. One of them, however, M. Felix Boyer, more sagacious than the rest, has sought to utilize the fruit, as the leaves had become valueless. He has found that ripe fruit pressed and fermented, furnished on distillation a pure and well flavoured alcohol, rivalling the best alcohol from grapes. 150 kilogrammes of the mulberries yield a hectolitre of juice, which yields on distillation 14 to 15 per cent. of alcohol.—*Revue de Thérapeutique Méd. Chirurg.*, March 1, 1866.

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Trichiniasis.—The *Wiener Medizinische Wochenschrift* states that the trichina disease has extended into Austria, and has appeared in Weigsdorf, Seitendorf, Königshain, Turnau, and Dornhemmersdorf.

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Rinderpest.—Much nonsense has been written about this disease, which has been prevailing so disastrously in Great Britain, and its importation into this country is so much feared. The following opinion of Prof. Gamgee, the highest authority on the subject, is worthy of attention. "Nothing but the most culpable ignorance could have led us to overlook the benefits of 'vaccination,' as a preventive against the cattle-plague, had any such benefits been observed and recorded in Russia. It is not vaccination but inoculation that is resorted to there, and this was first practised in England one hundred and twenty years ago. No sooner had Dodson taken up the idea from the inoculation for human smallpox, than the Dutch, Danes, and Germans conducted numerous experiments, and often with the result of losing only one per cent. of the inoculated animals. Skilled inoculators came in vogue. Many were their failures, loud were the complaints of the farmers in this and other countries; but nevertheless early in the present century, Walz and others suggested that all the calves in Russia should be inoculated, so as to render the entire horned stock of the country insusceptible to attacks of the plague. Prof. Jessen, of Dorfat, has very ably advocated the practice of inoculation in the Russian Steppes, and published a pamphlet on the complete extirpation of the rinderpest by this means in 1852. At that time a commission had been appointed by the Czar to advise as to the best means to be adopted for the prevention of the cattle-plague, which led to an annual average loss of 19,000,000 roubles. The only recommendation made by this commission was in favour of experiments as to the value of inoculation. Prof. F. Unterberger was sent to Southern Russia, and he turned his attention to cultivating lymph by passing it through the systems of animals, so as to modify its character, and he specially chose lymph from animals which indicated an eruption on the mammæ and mouth, an eruption which the history of rinderpest taught him had been more common when the successful inoculations of the last century were practised than it had been of late. The experiments which thus began in 1853, have been continued up to the present time, and I have in my possession all the reports published in the German language, and copious extracts from those published in Russian. The results arrived at are differently estimated by different individuals. Some still believe in the development of a great system of inoculation similar to that adopted for the vaccination of an entire population for the prevention of human smallpox. I am one of those who have no faith in the practice, and the ablest *résumé* yet published, which has but recently left the printer's hands in Dorfat, affords evidence of the hopeless nature of the task imposed on the Russians by those who believe that no horned animal of the Steppes should leave the supposed native haunts of the rinderpest without having been successfully inoculated.

"Vaccination, as now practised far and wide in this country, is another of the blunders of the present day. Rinderpest is rinderpest, and not smallpox. It is different from every other known disease of man or animals, though as a contagious fever it presents characters which some have regarded as not unlike those of typhus, typhoid fever, scarlet fever, or smallpox. It is none of these, and I regret that hasty conclusions as to points of analogy between rinderpest and variola have led to many fruitless and expensive experiments which have only served to direct people's minds from the one and only system by which such a malady can again be banished from our soil."—*British Medical Journal*, Feb. 3, 1866.

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The Cattle Plague.—This disease at the latest accounts was on the decrease in Great Britain.

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I am much pleased with the work; during the winter, I advised the class to use it, and regret that it only arrived in this city just at the close of the term. I shall employ it as a text-book.

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I shall take great pleasure in recommending this work as a text-book in our college, and also for the libraries of the profession generally. It is a timely and absolutely indispensable contribution to the literature of the profession.

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I consider "Flint's Principles and Practice of Medicine" as the best book upon the subject, that has yet come under my notice, for many other reasons as well as the one above mentioned. I shall most surely recommend the work to my class, and only wish every member had a copy.

From Richard McSherry, M. D.,

Prof. of Practice of Medicine, Univ. of Maryland.

I am much pleased with this work, and I take pleasure in recommending it to the students who attend my lectures.

From Israel T. Dana, M. D.,

Professor of Theory and Practice of Medicine, in Bowdoin College, Brunswick, Me.

On examination I am much pleased with the book, and shall warmly recommend it to our college students, next month, when my lectures at Brunswick commence again.

It presents a brief, but concise and reliable summary of those pathological and therapeutical views that are most generally accepted by the profession at the present time; and consequently it is well adapted for a text-book in the hands of students. It will also form a valuable addition to the library of the practitioner.—*The Chicago Medical Examiner*, April, 1866.

The Practice of Medicine of Prof. Flint is, undoubtedly, a most excellent work, and is much better suited to the special needs of the American student and practitioner than any other accessible to them. We predict for the book a very great, and, as we believe, well deserved popularity.—*Cincinnati Journal of Medicine*, March, 1866.

Contains all that has recently been added to our knowledge of this department of medicine.—*Detroit Review*, April, 1866.

Henry C. Lea, Philadelphia.